Fact and Fiction in Economics

Models, Realism, and Social Construction

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1 The factuality and fictionality of the "dismal queen"

Economics is a contested scientific discipline. Not only are its various theories and models and methods contested but, remarkably, what is contested is its *status as a science*. This becomes evident as soon as we think of some of the popular nicknames used of economics – such as "the dismal science" and "the queen of the social sciences."

Suppose we take one of the characteristics of science to be the capability of delivering relevant and reliable information about the world. Suppose furthermore that this is not just a capability, but also a major goal and actual achievement of whatever deserves to be called by the name of "science." How does economics do in this respect? This question is about as old as economics itself.

Many of those who are unimpressed think of economics as an arrogant and ignorant discipline, driven by methodological values that have little or nothing to do with the goal of delivering truthful information about the real world – values such as mathematical elegance and professional status. They might say that while economics may be the queen of the social sciences in regard to mathematical rigor, it is a failure in so far as its contact with the real world is concerned. Economics is largely a matter of formalized thin fiction and has little to do with the wonderful richness of the facts of the real world. It is the "dismal science," as Thomas Carlyle once put it.[1]

The expression "dismal science" seems to have grown in popularity – perhaps for reasons such as the new debates over the present and the future of economics, the current relaxed rhetorical atmosphere that favors fancy language, and, importantly, the ambiguity of the expression. The expression "dismal science" has many connotations. The most general and entirely useless one derives from its use as a tool for denouncing bad economic reasoning or an economic idea that one does not like. One of the more specific and familiar connotations relates to the Malthusian-type anticipations of a gloomy future, based on the presumed

fact of diminishing returns. Another relates to a depressing awareness of the "economic necessities" that govern social life in the form of budget constraints and trade-offs of various sorts. A related connotation refers to a heartless attitude towards human suffering, often attributed to the proponents of free market economics. Yet another relates to the narrow focus on calculative greed and its consequences as shaped by the values of money and the market, while being blind to social norms, customs, emotions, and the moral strings of personal relationships, thus missing major facts of economic reality. The final connotation is connected to the alleged impotence of the theoretically narrow and inward-looking academic economics in explaining, predicting, and controlling the functioning of the complex economic system – for example, in anticipating and helping prevent major economic crises. It is the last two connotations – economics missing important aspects of economic reality and its autistic impotence with respect to real-world issues – that are the most relevant to the main themes of this volume.

Other people, most notably many practicing economists, disagree on the pessimistic diagnosis of economics – or at least of their own favorite part of it – as "dismal." For them, economics is the queen of the social sciences, and this is so not only because of its superior mathematical rigor. They believe that the best of economics is driven by a keen interest in real-world issues and policy-relevance, and that it is capable of delivering insights and important information about economic reality: or at any rate more relevant and reliable information about economic issues than any other intellectual endeavor. These people – if they were methodologically enlightened – might say that it just appears as if economics deals only with fictions: the fictitiousness of economics is itself a fiction. In fact, economics – or at any rate a sufficiently large part of it – is very much a respectable fact-oriented scientific discipline. This fact about economics is easy to overlook, for the simple reason that the relationships between economic theory and reality are quite convoluted and hard to monitor: by necessity, reality is indefinitely complex, while theory is simple. Carlyle missed this because he did not understand that "all science is 'dismal' to the artist" as Schumpeter once put it (1954, 410).

The controversy around the "dismal queen" is old. In 1819, Simonde de Sismondi put forth a complaint that sounds very familiar today: "We see political economy adopting a more sententious language, enveloped in calculations increasingly difficult to follow, losing itself in abstractions and becoming, in every way, an occult science." One and a half centuries later, similar appraisals were put forward by many prominent economists. Indeed, the early 1970s witnessed a barrage of critical assessments from among the highest ranks of the economics profession: fellow economists were charged with "continued preoccupation with imaginary, hypothetical, rather than with observable reality" (Leontief 1970, 1) and for working with theories and models "built upon assumptions

about human behavior that are plucked from the air" (Phelps Brown 1972, 3). More specifically, the criticism was voiced that "these assumptions are frequently made for the convenience of mathematical manipulation, not for reasons of similarity to concrete reality" (Frisch 1970, 162). As a consequence, there "now exist whole branches of abstract economic theory which have no links with concrete facts and are almost indistinguishable from pure mathematics" (Worswick 1972, 78). These statements are manifestations of what Hutchison (1977) dubbed "the crisis of abstraction."

Ronald Coase's attack on what he calls "blackboard economics" is on largely similar lines. Coase suggests tracing this approach back to Joan Robinson's *The Economics of Imperfect Competition* (1933): "This new theoretical apparatus had the advantage that one could cover the blackboard with diagrams and fill the hour in one's lectures without the need to find out anything about what happened in the real world" (Coase 1993a, 51). Coase complains that "when economists find that they are unable to analyze what is happening in the real world, they invent an imaginary world which they are capable of handling" (1993a, 52), and summarizes his account like this: "What is studied is a system which lives in the minds of economists but not on earth. I have called the result 'blackboard economics' " (Coase 1993b, 229). Blackboard economics, so characterized, looks like sheer fiction and not in the least a factual enterprise. The famous discovery by Arjo Klamer and David Colander (1990, 18) appears to confirm Coase's worry: the economics students on the most prominent graduate programs at US American universities believe that being excellent in mathematics and skillful in puzzle-solving (on the blackboard, we might add) are important for success in economics, while having a thorough knowledge of the economy is regarded as unimportant for success.

In their discussion of what they call the "crisis of vision" in economics, Heilbroner and Milberg (1995) share these concerns. They argue that up to the post-Keynesian period – roughly up to 1970 – economics was characterized by analysis based on a vision of social reality and therefore by "its continuously visible concern with the connection between theory and 'reality.' By way of contrast, the mark of current economics is its extraordinary indifference to this problem. At its peaks, the 'high theorizing' of the present period attains a degree of unreality that can be matched only by medieval scholasticism" (1995, 3–4). Heilbroner and Milberg argue that, especially since the rational expectations revolution, there has been an "inward turn" away from real-world concerns and towards mere intellectual games amongst academic economists.

In this volume, the critical voice is Mark Blaug's (see also his earlier falsificationist account in Blaug 1980). In chapter 2, he laments the illness of formalism that he believes dominates economics and has turned it into a policy-irrelevant academic game. Special blame is put on general equilibrium microeconomics after the Arrow–Debreu proof in 1954, on the more recent fascination with

game theory, and on New Classical macroeconomics. Economists have lost their interest in tackling real-world issues, and some of them find justification for their attitudes in postmodern meta-theories that question the sensibility of notions such as the real world and its theoretical representation. Realism is the advisable alternative to help reorient economics, maintains Blaug.

In response to charges of the above sort, some practicing economists have taken on the task of defending economics as a fact-oriented discipline while blaming the critics for being uninformed about what is going on. Some argue, in diametric opposition to the critics, that in the last thirty years or more, economics has become more, rather than less, fact-oriented. A few prominent and representative illustrations will suffice to highlight the major themes in these arguments.

With a long career behind him, Robert Solow (1997) explicitly denies that mainstream economics has lost touch with reality. He recognizes a major change in economics from 1940 to 1990, but his diagnosis is decisively more moderate than that of the more radical critics: economics has become "a self-consciously technical subject, no longer a fit occupation for the gentleman-scholar" (1997, 42). Solow suspects that this may have led some observers to adopt the misconception of a discipline unconnected to real-world issues. Here we should add that this conclusion may require another premise, namely the observation that economics is a discipline without popularizers who would bridge the gap, in the minds of the lay audience, between forefront technical research and the pressing economic issues of the day (Krugman 1998, 8). Solow admits that there is a small minority of "formalists" in the economics profession, and that they are mainly writing to one another. Most of economics is not a matter of formalist fiction but rather model-building, "which is an altogether different sort of activity" (Solow 1997, 43) – more on this in a moment. The crux of the matter is that economics has become *technical* rather than "formalistic, abstract, negligent of the real world . . . Far from being unworldly, modern model-builders are obsessed with data" (Solow 1997, 57). If there is a problem, it is that there is a shortage of relevant data, and that sometimes model-builders keep building their models without adequate evidential checks-ups.

Another recent defensive voice is that of William Baumol (2000). In his assessment of the achievements of the economics of the twentieth century, he argues that, throughout this period, economics has made significant progress in what it offers to practice: "advances in empirical work and application of theoretical concepts to concrete issues of reality are where one can find the most distinct advances beyond the state of knowledge at the beginning of our century" (2000, 10). Baumol acknowledges that this observation cannot be extracted from economics textbooks that to a large extent fail to reflect relevant developments in actual frontline research. In his view, these developments stress the importance of rigorous data analysis and the interdependence between

theory and data: "we have grown increasingly uncomfortable with theory that provides no instruments for analysis of the facts and no opportunity for empirical testing" (2000, 26–27). The employment of sophisticated mathematical techniques and drastic theoretical simplifications promote, rather than hinder, success in applied research that endeavors to support practice. The basic image of economics Baumol is suggesting is one of a discipline responding, in a systematic and rigorous fashion, to demand based on concern with practical real-world issues. It is an image of a fact-oriented discipline.

Representative of a younger generation, David Kreps (1997) offers further nuances to the largely optimistic picture. Kreps perceives a strong trend, in the last thirty years or so, towards a broadening range of research issues that are tackled in an empirically sensitive fashion by economists who are increasingly willing to reconsider the assumptions of their theories. Like Solow and Baumol, Kreps points out that there is an increasing body of data available to economists, and that they are increasingly prepared to produce more data themselves, for example by way of experimentation. He also indicates the growth of two-way interaction across traditional disciplinary boundaries with biologists, sociologists, and psychologists whereby economists learn from these fields. In microeconomics, Kreps identifies two trends, one more radical than the other. The less radical trend consists in relaxing "contextual" assumptions such as large numbers and anonymity of agents, shared information, and static analysis, and replacing them by small numbers interaction, asymmetrical information, and nontrivial dynamics. This is the main current in the new microeconomics. The more radical trend consists in relaxing one or more of the "canonical" assumptions of far-sighted rationality, purposeful greed, and equilibrium. This trend is understandably weaker as it challenges the canon and meets with more resistance from the established paradigm. Even though the canon is admittedly empirically deficient, the move away from it will be impeded by the (still) relative shortage of adequate empirical data and the possibility of tweaking the true-to-the-canon models on the face of almost any evidence.[2] What emerges from this is a qualified optimism about economics as a factual discipline.

In chapter 3 of this volume, Partha Dasgupta joins the camp of those who have set out to defend economics, motivated by a sense of social responsibility to defend an unjustly criticized discipline. Just like Solow, Baumol, and Kreps, Dasgupta claims that, in the last quarter of a century, economics has become more rather than less factual. While Baumol warns against just looking at textbooks, Dasgupta warns against just listening to what economists say about their work: both recommend looking at what they do in their research. Dasgupta explicitly launches a counterattack against the version of discontent put forth by Heilbroner and Milberg. By citing a number of examples in recent research, he argues that economics has moved away from grand theoretical issues towards

small and sharp applied issues, and that this has helped economics become increasingly factual.

I have listed just a small selection[3] of representative assessments of economics, and the clear picture that emerges is that there is no clear picture. Opinions diverge as to whether economics is on the right or wrong track, and, if on the wrong one, when exactly the sinning started: in the early 1930s, early 1950s, or early 1970s? Given the role and status of economics in university education, in policy, and in our culture at large, the radical disparity of these commentaries must be found very confusing, if not alarming. What to make of such striking differences in the assessments of economics? Whenever one comes across with such polarized claims, it is time for further questions and some conceptual scrutiny. This is where a little help from one's methodology friends is welcome, and this is where this volume sets out to offer some community service. Things will turn out to be much more complex than the most simplistic statements suggest.[4]

The first easy observation is that "economics" is a dangerously aggregated notion that hides a lot of variety and diversity behind it. One takes big risks by maintaining that economics is like this or economics is like that – for the simple reason that there is no one homogeneous "economics" about which one can justifiably make straightforward claims. A more differentiated approach is advisable. Statements should be made about particular branches of economics during particular spans of time being factual or fictional in carefully specified respects. Another obvious qualification is that the disjunctive "fact or fiction?" is misleading. The right configuration is the conjunctive "fact and fiction" – this latter serves as the title of this volume. *Any* scientific discipline combines fact and fiction, and there are many kinds and degrees of factuality and fictionality.[5] Finally, whenever one attributes fictionality or factuality to something, one has to be very clear about what exactly this something is – a concept, an assumption, a model, a framework, a piece of data, a metaphor, a graph – as well as what one means by "fact" and "fiction."

Philosophers have offered a number of rival accounts of both fact and fiction. Economists and others, on the other hand, use these notions without analyzing their precise meanings. In a volume like this, bringing together a variety of themes, approaches, and perspectives, there cannot be a precise account of the notions of fact and fiction, unifying the contributions. We need to be content with somewhat intuitive and simple ideas. These notions can be linked to the issue of realism (of which more will be said in chapter 4). One can be a realist about the world and about theories of that world. Take *T* to be a theory, model, or assumption related to chunk *S* of the world. One is a realist about *S* in relation to *T* if one believes that *S* exists independently of accepting, believing, or uttering *T*. One is a realist about *T* in relation to *S* if one thinks that *T* and its constituents refer to *S* or that *T* in addition truly represents or should truly represent *S* – where truth is likewise independent of whether *T* is accepted, believed, or

uttered. These definition sketches imply that, for example, the observability of an object and the testability of a theory are conceptually unconnected to realism.

Facts are what is the case, they are what make true statements true. A true statement is true because it stands in a suitable relation (such as that of correspondence) to facts in the world. Many economists believe that it is a fact about inflation that it is a monetary phenomenon. The link between facthood and truth then suggests that to say, "it is a fact that inflation is a monetary phenomenon" is to say, "it is true that inflation is a monetary phenomenon" (which, the redundancy theorists of truth will controversially add, is nothing else but to say, "inflation is a monetary phenomenon"). On this view of facts, facts are objective features of the world that serve as the truth-makers of true statements: if "inflation is a monetary phenomenon" is a true statement, then what makes it true is the fact that inflation is a monetary phenomenon. Some philosophers are concerned about whether there is sufficient distance between fact and truth, but for our purposes it is enough if we just take facts of the economy to be objective features of social reality that are not constructed in the intellectual games economists play. What counts as a fact and what counts as true in a community of scholars is socially constructed, whereas what is a fact and what is true, is not. Such a simple distinction will satisfy some unqualified realist intuitions.

One can attribute fictionality both to objects and to representations. We may say that an object is fictional where its existence and the truths about it are dependent on particular descriptions of it. Just like Robinson Crusoe's existence and any truths about him are dependent on Daniel Defoe's descriptions, the existence of *homo oeconomicus* and truths about "him" may be dependent on the various assumptions used by economists to describe the economic actor. One may then regard a representation such as a model or its constituent assumptions as fictional if it is about such fictional objects. If one thinks there are nonfictional real objects in the world as well, one may call a representation fictional if it is not taken to refer to any real objects, thus is not used for making assertions or conjectures about the real world. It lacks factual truth-value altogether: it is factually neither true nor false because it is about nothing real. Another possibility is to consider a representation fictional because it is false or radically false when interpreted as an assertion or conjecture about the real world. One then proceeds to study the real object *as if* it were as represented. Both of these ideas seem to appear in the commentaries of economic models: these models are claimed to be fictional in being radically false or in lacking truth-value altogether.

* * *

These issues can be approached from at least three perspectives, from the point of view of three questions. (1) *How do economic models function*: How do economic models and theories relate to the world? This question, too, has many

facets and thus falls within the semantics, epistemology, and methodology of economics, addressing questions of truth, knowledge, and methods of testing. (2) *How does the economy function*: What is there in the social world that will be causally or constitutively relevant to the functioning of the economy, or to the occurrence and shaping of economic phenomena? This is a question in the ontology of economics.[6] (3) *How does the academic discipline of economics function*: What is its structure of institutional constraints and behavioral incentives that shapes the endeavors of economists? How does the "industrial organisation" of economics enhance or hinder its fictionality and factuality? To answer these questions, one has to study the institutions of economics – the rhetoric, sociology, and economics of economics.

In actual practice, these are not fully separate perspectives, but for the purposes of this volume, the chapters are arranged in these three categories. These three perspectives have been characteristic of my own work, and I am delighted that the invited contributions appear to fall within this scheme. The scene is set by raising some of the key issues in the three chapters in part II of the volume. The six chapters in part III address question (1), asking how models link with reality. Question (2) about the constitution of economic reality is addressed by the five chapters in part IV. Finally, question (3) about the institutions of economics is the theme of the last three chapters in part V of the book.

2 Economic models

To do economics is to do modeling. In assessing the truth of this claim one had better be attentive to the ambiguity of "model." On a narrow sense of "model" – a notion of model defined in terms of mathematics – the claim may have a great deal of truth in it, even though it may be taken to exaggerate with misleadingly restrictive implications (such as "you are not doing economics if you don't build mathematical models"). On a broader sense of "model" – model as selective representation – all of economics was, is, and will be, a matter of modeling; and there is nothing peculiar about economics in this respect, in comparison to cosmology, chemistry, criminology, and casuistry.

If there is a puzzle about modeling, it is that economists build models that depict model economies that may appear to bear little or no resemblance with the real world. For outsiders, such as journalists, beginning undergraduate students, and many other social scientists, it may appear as if economists are living in a dream world of their models, in an imaginary world of fiction that they themselves have designed. The challenge for economists and economic methodologists alike is to analyze the ways in which models could convey, or fail to convey, truthful information about the facts of real economies.

Above, I cited Solow's remark that economics is engaged in model-building that is an activity different from what "formalist" economists do. Indeed,

model-building at its best can be construed as fact-oriented activity that takes as its objective to isolate key causal dependencies in reality: "The idea is to focus on one or two causal or conditioning factors, exclude everything else, and hope to understand how just these aspects of reality work and interact . . . modern mainstream economics consists of little else but examples of this process" (Solow 1997, 43). This is to say that modern economics is a matter of using the generic method of isolation, of inclusion and exclusion, of focusing on key elements and neutralizing the rest, of simplification and idealization. Models involve idealizing assumptions that are strictly false but serve the purpose of simplifying the problem attacked by excluding or neutralizing many factors that might be expected to have an impact on the outcome of an actual process. Such false assumptions help isolate some key dependencies for closer inspection. While laboratory experiments accomplish such isolations by way of causal manipulations of actual situations, the isolations of a model-builder take place in the theoretical sphere as thought experiments. Models are (among) the economists' laboratories. (See Mäki 1992a.) As Solow suggests, "A good model makes the right strategic simplifications. In fact, a really good model is one that generates a lot of understanding from focusing on a very small number of causal arrows" (Solow 1997, 46). A model isolates one or a few causal connections, mechanisms, or processes, to the exclusion of other contributing or interfering factors – while in the actual world, those other factors make their effects felt in what actually happens. Models may seem true in the abstract, and are false in the concrete. The key issue is about whether there is a bridge between the two, the abstract and the concrete, such that a simple model can be relied on as a source of relevantly truthful information about the complex reality.

Since realists are friends of truth, they want to have models that provide truthful representations of economic reality. The challenge is to reconcile this goal with the intrinsic feature of models that they contain a lot of falsehood. This is too big an issue to be discussed here in any satisfactory detail and comprehensiveness, but let me make a brief remark about the important notion of representation. Virtually any objects can serve as models of something else, and such objects can be of various kinds: models may be material, linguistic, and abstract objects; they can take on the form of concrete analogues, graphs, experimental designs, idealized thought objects, systems of mathematical equations, and so on. In each case, we may think of a model, *M*, as a simple system used as a representation of something else, a more complex system, *X*, in two senses. First, *M* represents *X* in that *M* is used as a representative of *X*. By studying *M* instead of *X* directly, one hopes to learn about *X*. One manipulates *M* by way of constructing, experimenting, calculating, and imagining, and so learns about the properties of *M*. Second, *M* represents *X* by resembling it in relevant respects and sufficient degrees relative to the use to which *M* is put. Thanks to this resemblance, the examination of *M* will convey information about *X*.

We may say that the two aspects of representation are interdependent in that *M* earns its justification as a representative of *X* by resembling *X*, or corresponding to it. Resemblance is a matter of relevant respects and sufficient degrees, and these are relative to the many possible uses of models as representatives.

This is far from a complete account of models, but should give some clues as to the variety of aspects involved in the issue of how models relate to reality (other questions deal with how models relate to theories and data, for example; see Hausman 1992; Morgan and Morrison 1999; Mäki 2001b). The intuitions behind judgments of the familiar sort, "this model is (un)realistic," are unhelpful – vague and devoid of implications concerning the adequacy of the model – unless made explicit along the several dimensions that are involved. Chapters 5–10 in part III of this volume offer further illumination on selected aspects of this conundrum, discussing different kinds of models and various ways in which they might have something to tell us about social reality.

In chapter 5, Robert Sugden argues that abstract and unrealistic models are able to provide true and important information about the real world. Using Akerlof's "lemons" model and Schelling's checkerboard model of racial segregation as illustrations, Sugden develops an account of how the imagined world of the model connects with the real world. We may say that on this account, good economic models satisfy both aspects of representation. In the model world, a cause brings about an effect, such as a regularity, or could do so. Other causal factors and connections, active in the real world, are not considered. Sugden suggests that the move from the model world to the real world is an inductive inference from claiming a connection in a highly simplified case to claiming it in real-world cases under various contingencies. Our confidence in this inference is based on the belief that the model worlds – such as Schelling's checkerboard cities and Akerlof's used-car market – are possible, that they could be real given what we know about how the world works. Such a possibly real model world is a credible world, and such a simple credible world is not an isolation of a small set of elements from the rest of the actual real world, or so Sugden argues. He suggests that the credibility of a model is a matter of *coherence* – a harmonious relationship between the assumptions of the model, and between the model and what we know about the causal structure of the world. This is an intriguing account that offers a way of thinking of good economic models as truthful representations of matters of fact. It gives rise to further questions, such as how the suggested inductive inference relates to analogical reasoning, and how the idea that credible model worlds are constructed relates to the idea of theoretical isolation.

In line with an old scientific and philosophical tradition and her own earlier work, Nancy Cartwright argues in chapter 6 that laws – empirical regularities of stochastic or nonstochastic kind – are not free-standing features of the world, they rather require a background structure that generates them.

These underlying socio-economic structures or chance set-ups ("nomological machines" as Cartwright calls them) contain things equipped with causal powers or capacities to bring about definite effects. This is the traditional nonempiricist ontology behind her account of models. A model is about a nomological machine or causal mechanism under highly stringent conditions such that strict lawlike regularities arise. The problem with economic models is that those idealized conditions hardly ever materialize. In the actual world, many causal mechanisms interact in an uncontrolled manner and thus fail to generate strict empirical regularities. Economic models are constructed in terms of very concrete concepts, close to everyday experience, which is why the models do not have precise deductive implications (do not yield "results," as economists would put it) without being engineered in just right ways by "hyperfine-tuning" – by imposing stringent idealized constraints on the models. This, Cartwright believes, makes them fit only with very special situations in the world, thereby radically restricting their scope of applicability. Situations that would satisfy the idealized conditions of the models, while common in laboratory sciences, are rare or nonexistent in the economic world where strict manipulation of causal factors is impossible. This is the pessimistic conclusion of her local realism. (See also Cartwright 1999.)

Cartwright's conclusion is based on premises that one may challenge. One could raise questions about her ontological framework, including her (empiricist) notion of law; her implicit view, as it seems to me, of the primary function of idealizing assumptions as determining conditions of applicability; her methodological views about the standards of science; and her views about what econometrics and economic models are all about. Sugden's account is not about econometric models, but it includes a resource that could be used to question an element in Cartwright's account: the idea of inference from fine-tuned but credible model worlds to real-world situations that do not satisfy the idealized conditions of the models. In chapter 9, Backhouse takes issue with Cartwright's arguments by suggesting that her pessimism about econometrics is due to her general and overly strict standards of science, and defends instead field-specific standards: precise and stable quantitative relationships do not occur in the domain of economics and should not therefore be required. Kevin Hoover's chapter 7 makes a similar point by suggesting that precision on the one hand, and reliability and scope on the other, come in degrees and that there is a trade-off between them: more precision, less reliability, and smaller scope; less precision, more reliability, and broader scope.

Both Cartwright and Hoover look at econometrics from a realist point of view, but they draw different conclusions, because their understanding of econometrics is different. For Hoover, econometrics is neither about measuring strict universal regularities that could serve as covering laws (as in Tony Lawson's account), nor about characterizing the causal powers of socio-economic structures

or nomological machines. Hoover's defense of econometric models is by way of being more modest about their goals. Econometrics is about observing nonobvious robust regularities. These regularities are not regarded as free-standing features of social reality, since they are believed to be generated by (and ultimately to be explained in terms of) causally powerful structures even though the econometric models do not describe these structures. In case more generality is claimed for the regularities, less precision will be imposed, while more specific models incorporating information about local circumstances can be used to make more precise claims. Hoover points out that while a theoretical model – highlighting perhaps just one mechanism – is highly fine-tuned, the corresponding empirical models incorporate the influences of several mechanisms and are more schematic, include vaguely defined variables, and are not tightly linked to the respective theoretical model. (See also Hoover 2001.)

Both Morgan and Backhouse emphasize the informal aspects of modeling. In chapter 8, Morgan argues that the use of models involves telling stories: one cannot fully describe and understand a model without understanding how it works, and narrative stories are integral elements in the working of a model. In particular, it is by means of these stories that models are linked with reality. This may be taken to imply the claim that one has to incorporate stories in one's account of models in order to avoid unnecessarily fictionalist views of economic models. Stories connect the abstract to the concrete by way of providing interpretations of mathematical formulas, explanatory questions and answers, supplementary causal chains, and other things. In his chapter, Backhouse argues that the term "story" as used by Morgan covers a number of distinct informal elements in the use of models and that more traditional terminology is better in capturing these elements – such as theory, interpretation, causal mechanism, problem, and its solution. Another issue has to do with what emerges from Morgan's account regarding the very concept of model. One may read Morgan's chapter as an account of the pragmatics of modeling – of the role stories play in the use of models for certain purposes. On the other hand, she also says stories are part of the identity of models. On the first reading, the uncontroversial point is implied that models don't *do* anything: models *are used to do* things. It is suggested that stories – or whatever one wants to call this heterogeneous set of items – are essential tools in using models. One can then raise questions about epistemic appraisal: for example, can a bad model be saved with a good story? On the second reading, stories are part of models, and models are indistinguishable from their use. In describing a model one describes its use which involves telling narratives. An obvious question to ask is how one is supposed to appraise the multitude of closely related but distinct models conceived as such conglomerates. Morgan's chapter is not unambiguous between these readings.

While acknowledging that economic models are not quite true, economists themselves frequently defend them as being close to the truth or as approximations to the truth. These are difficult notions that Karl Popper's doctrine of

verisimilitude failed to analyze, but they have kept some analytical philosophers of science busy with a headache. In chapter 10, the leading expert on these notions, Ilkka Niiniluoto, discusses some of the key ideas (for a survey of some of the relevant philosophical literature, see Niiniluoto 1998). He focuses on the idea that models may be intended to highlight the key dependencies in the domain modeled, and discusses it in terms of truthlikeness, counterfactuals, idealizing assumptions and their relaxations, and reference. He defends a realist account of theories and models that involve false assumptions. Many of the insights suggested by Niiniluoto will be put on the research agenda to be exploited by philosophically minded students of economics.

The chapters dealing with economic models generously offer many examples of notions that can be used to enrich our instruments of assessing the factuality and fictionality of economics, and to do this in a manner that is more refined than what one encounters in most commentaries about the credentials of economics. The list would include concepts such as theoretical and empirical model, story, regularity, causal mechanism, causal power, precision, reliability, scope, robustness, counterfactual, idealization and its relaxation, horizontal and vertical isolation, and kinds of truthlikeness.

3 Economic ontology

If modeling were just a matter of a formal exercise with a goal of showing that a stylized fact can be derived from a set of premises, then economics would be an all-too-easy intellectual game. For any given stylized fact, there is an infinite number of possible models that entail it in a logically appropriate fashion. Drastic selection is required to sort out a tractable set of models that is regarded as worth the economists' attention. For a selection of a choice set of models, constraints are needed. Some such constraints are based on economists' and others' beliefs about the constitution of social reality. The imposition of such constraints will delimit the choice set, the range of minimally plausible models considered as candidates for further scrutiny.

We have entertained the possibility that good economic models are about economic reality and purportedly represent its properties, its structure, and its functioning. Obviously, the prospects of modeling are dependent not only on the properties of the models put forth by economists, but also on the properties of economic reality. Economists, other social scientists, philosophers, as well as business people and other economic actors hold (pre-model or extra-model) views about various fundamental properties of the economy. These views can be characterized as (rival and complementary) parts of economic ontology. Such ontological convictions characteristically remain imperfectly elaborated and they tend to be taken for granted without much or any explicit argument. Among such convictions are the individualist doctrine of the individual as the fundamental building block of society – as well as the anti-individualist views that

dispute this idea; various "models of man" that constrain views of what counts as rational behavior; views about whether values and emotions are causally relevant factors in economic processes; implicit conceptions of the free will that shape views on, say, what counts as involuntary unemployment; the constitutive metaphors of the economy as a clockwork or as an organism; the range of types of institution one regards as playing major causal or constitutive roles in economic processes; the belief that the market is, or is not, a self-coordinating system; the related pre-analytic beliefs about the relative importance of market failure and government failure; the belief that there are, or are not, sufficiently robust macro regularities in the functioning of the economic system that can be used for controlling it by way of deliberate policy; conceptions of whether the society is a unified system governed by a small number of dominant principles; the view that statistical correlations are generated and sustained by socio-economic structures; the various views of lawlikeness as what regularly happens, as what would happen in certain conditions, and as what tends to happen.

Such ontological convictions held by economists and others appear at different levels of generality. Some social ontologies are about the constitution of society in general, raising and answering questions about the possibility and existence of social order (see Giddens 1984; Pettit 1993; Tuomela 1995). Some others are concerned with the economic realm more narrowly, dealing with the ontology of economic agency, the market mechanism, and economic aggregates, among other things. Such convictions function variously as constraints on acceptable economic theories and models and explanations. Sugden's suggestion that what makes a model world credible is coherence with what we know about the *w*ay the *w*orld *w*orks is in line with the "www constraint" on acceptable theories and models (Mäki 2001a). For example, one may hold the conviction of ontological individualism (only individuals are real, hold beliefs and goals, and act) and insist that all acceptable, non-ad hoc theories should be derivable from suitable microfoundations. One may also endorse a general causal process ontology and insist that it is a mark of good economic theories that they give an account of causal processes – rather than, say, just descriptions of states of equilibrium. Or one may hold a more specific view of a given economic order and require that acceptable theories be respectful for the fundamental characteristics of that order. The Heilbroner–Milberg (1995) thesis exemplifies this latter type of constraint. They argue that adequate economic theories are consistent with a specific vision of capitalism as a complex social system characterized by capital accumulation as the driving force; the market as the organisational mechanism of allocation; and the division between a private and a public sphere as the dominant administrative principle (1995, 106–109). They claim that much of current economics has lost touch with reality because it has lost connection with such a vision – or because it does not meet a specific "www constraint," as we may put it.

Economic ontology may be partly based on the models economists put forth. But it inevitably draws – explicitly or implicitly – from other sources as well, such as other social sciences, social actors' experience, religious convictions, and philosophical categories and arguments. Once an economic ontology is in place, it variously shapes the models economists build. It is not claimed that such a general economic world view – a system of general conceptions about the economic realm – uniquely determines the form and contents of economic models. There are two reasons why unique determination does not take place. One is the unavoidable slack between a general ontology and any specific economic model. The relationship between the two levels of generality can at most be one of constraining: economic ontology constrains a feasible set of economic models. The second reason is that even though sometimes specific economic models and a more general economic ontology are in harmony with one another in the sense of one meeting the constraints imposed by the other, at other times there is a tension between the two. In the latter case, the economist's deeper convictions may be in tension with the models she holds: for various reasons, such as the limitations of available formal techniques, the models built and held are not (yet) consistent with the general ontology. In both cases – the cases of harmony and disharmony – the role of economic ontology may be crucial. In the harmonious case, the form and contents of a set of models fit with the ontological convictions, thus giving assurance that the models are right. In the disharmonious case, the mismatch between models and ontology may function as a driving force behind a evolving sequence of models and modeling techniques: economists are inspired to look for more adequate techniques and seek to build models that cohere better with their underlying convictions about the constitution of economic reality.

Some commentators suspect that many economists are unconstrained by such deeper convictions, or at any rate by any such systematic ontological visions about the economy. Schumpeter (1954) referred to the "vision" of economics, together with economic "analysis" the history of which he set out to write. Heilbroner and Milberg (1995) adopt this distinction and argue that there is a crisis of vision in modern economics. Their notion of "vision" comes close to our idea of economic ontology: "By vision we mean the political hopes and fears, social stereotypes, and value judgments – all unarticulated, as we have said – that infuse all social thought, not through their illegal entry into an otherwise pristine realm, but as psychological, perhaps existential, necessities" (1995, 4). In comparison to the notion of economic ontology as used here, this formulation stresses the normative elements and perhaps underplays the descriptive convictions involved in a vision – while rightly emphasizing the implicitness and inescapability of such fundamental convictions. Heilbroner's and Milberg's worry about modern economics is "the widespread belief that economic analysis can exist as some kind of socially disembodied study" by

which they mean analysis without vision (1995, 6). This, they argue, has led to "the extraordinary combination of arrogance and innocence with which mainstream economics has approached the problems of a nation that has experienced twenty years of declining real wages, forty percent of whose children live in 'absolute' poverty, and which has endured an unprecedented erosion of health, vacation, and pension benefits . . . Once the dismal science, it will become the irrelevant scholasticism" (1995, 6, 8).

It is not clear how exactly one should read such complaints. Let us suppose we can distinguish between the descriptive and normative aspects of a vision and of a model. Let us further suggest that the notion of real-world connection includes, among other things, the notions of *reliability* and *relevance*. In assessing analytical models, reliability is a property of the descriptive component, while some aspects of relevance are based on normative considerations such as those that concern the moral or political significance of the issues that are selected for attention. Assessments of reliability make claims about the descriptive performance of analytical models in regard to those selected issues. In these terms, what is the thesis of the crisis of vision and its harmful consequences for the real-world connection of economics? One may read it as the claim that, without the guidance of a vision, economists fail to tackle relevant real-world issues. Or one may read it as claiming that whatever issues are addressed, the information conveyed about them is not reliable. One may then try to combine the two claims by suggesting that if analytical modeling becomes unconstrained by considerations of relevance, it is inclined to become just an inward-looking academic game and lose touch with the real world and hence the ability to convey reliable information about it.

Unsurprisingly, ontological convictions or visionary views tend to be at least as contestable as the models that economists hold. James Buchanan (1999) agrees with Heilbroner and Milberg on the claim that economics has lost its vision, but he has a different conception of the contents of the appropriate vision. Buchanan believes there is a coherent explanatory vision of "the inclusive structure of social interaction . . . informed by an understanding of the principles of operation" (1999, 2–3). This vision can be traced to Adam Smith and other classics and their insight that people seek to better their own position and that there are mutual gains from trade. At the core of this vision is the notion of value arising from the exchange process in the market. Economics has taken the wrong turn "when value, in any meaningful economic sense, is presumed to exist independently of market evaluation through exchange processes . . . As the superficial analytical sophistication increased, the formal structure of neoclassical economics somehow lost its behavioral moorings" (1999, 6–7). While Heilbroner and Milberg believe that Keynes was on a relatively right track regarding visionary matters, what Buchanan calls the "Keynesian aberration" is based on misunderstanding this classical ontological vision.

Other disagreements about ontological visions may concern their degree of coherence or systematicity and the ways in which they are revised. One may argue that a systematic, unified vision is required for establishing a relevant real-world contact for models; someone else may hold a vision according to which the world itself is fragmented – a unified vision would be a distortion of the facts. One might also admit that there are occasional "crises of vision" but that this does not imply any serious deficiency of factual orientation: an economist may be deeply concerned about real-world issues, but believe that a new vision is needed and furthermore that the path to such a revised vision goes through fragmented models.

The properties attributed to economic actors are among the key elements in any economic ontology. As we know, *homo oeconomicus* has a long and varied history during which he (*sic*) has been equipped with a variety of objectives, epistemic and other capabilities, attitudes regarding others, and so on. Most economists have granted that real human beings are not quite like that, but argue that depicting them in those terms is scientifically justified. There seems to be a tension between the ontological convictions of economists and the assumptions of actors they employ. Some have sought to resolve the tension by adopting an instrumentalist position: *homo oeconomicus* is just a fiction, but it serves well the goals of inquiry such as the organization or manipulation of empirical facts. The interesting question is whether there is a realist interpretation of *homo oeconomicus*.

In chapter 11 in part IV, Philip Pettit argues that if we construe *homo oeconomicus* in terms of self-regarding desires, the resulting creature runs counter to commonsense experience that takes people to recognize loyalty and fair play, kindness, and honesty. It appears that there is an empty "black box" at the origin of economic behavior, with no mechanism inside it. Pettit argues this is an appearance only, but to see what is in the box, we need to have a more refined ontology. Self-regarding desires are real in a special sense – they are virtually real. People are ordinarily driven by culturally framed routines of friendship, obligation, and so on. Only in situations where an individual's interests are violated or served below some tolerable level of aspiration, will the self-regarding desires and deliberations become activated. On this ontology of social actors, self-regarding desires are not actual causes but rather "standby causes" or potential causes that are triggered in those special situations. Such virtually real standby causes have explanatory power of a certain limited kind: they explain the resilience or robustness of behavioral patterns. This is how the conventional economic assumptions can be aligned with commonsense beliefs about human behavior, even outside of the traditional economic realm. Pettit also shows that the same argument can be used to salvage functionalist theory, a popular target of individualist criticisms. The claim that functionalism is flawed in offering an empty "black box" devoid of any selection mechanism

is not sensitive to the more refined ontology of virtual selection. A selection mechanism is virtually real and becomes activated in special situations and thus helps explain the resilience or modal persistence of certain important social institutions. As Pettit says, in both cases, the "black boxes" are empty in one sense and not empty in another. Fact *and* fiction.

Many proposals have been recently made to ascribe richer contents to the box of economic actors. In chapter 12, Shaun Hargreaves Heap suggests that judgments of self-worth are to be incorporated as ontological constraints on theories. The idea is that it is an important feature of social reality that people seek to make sense of their social lives: people give accounts of the reasons and worth of their social behavior. In this sense, the social reality encountered by an economist is pre-interpreted by social actors themselves (this view is sometimes called "existential hermeneutics" or "hermeneutical ontology"; see Lavoie 1991). Hargreaves Heap suggests that the standards of judgments of self-worth are shared by others, hence they form a common culture that is external to particular individuals. Such common cultures constrain the theories agents can use in interpreting their actions from the point of view of self-worth; for example, a problem with the preference satisfaction model is that preferences are not publicly accessible. A model with wealth-seeking will do better, in particular in contemporary Western culture, especially when supplemented with considerations of cooperation, fairness, shame, and embarrassment. Supposing one requires some sort of continuity between the theories the agents use and those that economists use, Hargreaves Heap turns out to have developed an ontological constraint on economic theories that will be welcome by hermeneuticists and realists alike – and that will avoid the hazards of relativism.

From a different angle, chapter 13 by Raimo Tuomela and Wolfgang Balzer pursues an account of collectivity and cooperation. Economics has not traditionally been strong on such notions, with manifestations of the prisoner's dilemma type. These notions also relate to the theme of social construction. There is an obvious sense in which social reality is constructed by people: we make the social world through our conceptualizations and interactions, attitudes, and acceptances. Yet, there are senses in which the social world is real. Tuomela and Balzer outline some aspects of a detailed account of how the social world is constructed (for a more comprehensive exposition, see Tuomela 1995). Collective acceptance is the key to the construction (including maintenance) of many social entities and properties. Squirrel fur counts as money in a society and a particular person counts as a CEO of a company because these things have been collectively accepted to be so. Collective acceptance can be, respectively, norm-based and agreement-based. Collective acceptance in the we-mode is by group members *qua* such members, while collective acceptance in the I-mode does not require such group orientation. Such distinctions give us different kinds and degrees of sociality or collectivity. The second section of the chapter compares

the ensuing goal notions with the concepts of public good and club good as used by economists. In addition to Tuomela's, other accounts of sociality and collectivity include those of Gilbert (1989), Pettit (1993), and Searle (1995). This body of literature will serve as a resource in the search for a more genuinely *social* ontology, and thereby more factuality, for economic theories and models (see Sugden 2000).

Social institutions and arrangements are constructed and maintained, but they also change, sometimes in a piecemeal fashion, sometimes abruptly. Mainline economics has not been very strong on the theme of social change. The identity of Friedrich Hayek's theory of cultural evolution – evolution of traditions of rules and norms, moral precepts and practices – as an economic theory may be an issue, and so is its correctness, but it is worth a closer look as a source of ontological insight. In chapter 14, Bruce Caldwell takes on the task by considering three classes of criticisms against Hayek's account: his idea of group selection as a mechanism of cultural evolution is inconsistent with his professed individualism; his epistemological pessimism about people's ability to constructively shape social institutions recommends against attempting to improve the constitutional framework of society; and his explanation of how cultural evolution occurs is incomplete. In response to these criticisms, Caldwell shows, first, that if Hayek is taken to endorse an individualist outlook at all, his is a broad "non-neoclassical" version of individualism that is consistent with group selection. Second, he also points out Hayek's ambivalence about the need of the external imposition of constitutional rules in addition to the endogenous establishment of informal norms and moral rules, and considers possibilities of resolving the tension between constitutional political economy and Hayek's epistemologically pessimistic critique of "rationalist constructivism." Finally, Caldwell admits that Hayek's account of cultural evolution is very incomplete and unrefined, lacking detailed ideas of the units of selection and the mechanisms of variation and selection. But if Hayek's intuitions are on the right track, the incompleteness of his account serves as an invitation to others to join in an interdisciplinary project of developing them into a serious theory that would support an ontology that is not only social but also genuinely dynamic.

Chapter 15 by Neil De Marchi is a study of the notion of "facts in the concrete" in J.S. Mill's writings. The chapter is a piece of historical inquiry, but the topic is timeless and very relevant for today's concerns and debates about economic theory and method. De Marchi's study has an ontological aspect – dealing with the ontology of facts in the concrete – as well as an epistemological aspect – how theoretical reasoning should relate to such concrete facts (thus the chapter cuts across the first and the second themes of this volume). The problem with using facts in the concrete in economic theorizing stems from their ontology: they are mixtures of the effects of a multiplicity of causes. These causes are hard or impossible to access independently owing to the

unavailability of effective experimentation. They cannot therefore be used as a secure basis for deriving general principles, nor for falsifying such principles. Mill wanted to ensure certitude for the statements of political economy as he thought this is required for its public credibility (this provides a link with the third theme of this volume). He believed the method appropriate for attaining this goal has to be some other than the use of history or experimental method. De Marchi traces Mill's development to the view of the *a priori* method of political economy. The solution is to start from the assumed laws of the human nature and to deduce their consequences for economic phenomena. The list of such "laws" relevant to the economic realm is short, as it isolates only a few of them from the full set: the desire for wealth and the capacity to judge the relative efficacy of the means to this end as well as the antagonizing principles of an aversion to effort and a preference for present enjoyment. This is not a complete list of factors driving real people, but it gives us an account that is closer to the truth about economic phenomena than any other equally simple alternative. And while experience of facts in the concrete cannot provide a secure source for general principles, it does constrain theories and explanations in important ways. We might say – and De Marchi might agree – that while sound economic theory does not capture facts in the concrete, it does capture facts in the abstract. Fact *and* fiction again. Phrased in various vocabularies, this line of thought is pursued by Cartwright, Sugden, myself, and others in the methodology of economics.

The chapters in part IV, devoted to economic ontology, highlight a variety of categories that are needed for expressing and refining the ontological convictions economists hold. These include virtual reality, standby cause, resilience, self-worth, common culture, kinds of collective acceptance, elements of cultural evolution, individualism, and facts in the concrete. Many more are needed to determine the complete set of ontological convictions that constrain and should constrain the theories and models accepted by economists.

4 The institutions of economics

The third perspective from which the issue of the fictionality and factuality of economics can and should be approached is the practice of research and communication by economists, and the ways in which this practice is conditioned by the institutions of economics. Such institutions are rules of the game: they consist of structures that relate to incentives and rewards, education and employment, publishing and expert consultation, agendas of topics, and standards of assessment. The institutions of economics shape the values and goals of practicing economists. Like other institutions, they are subject to change. The question here is about the direction to which the academic (or nonacademic) institutions at any given time and place guide model-building. Many of the